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Roman wellbeing and the economic consequences of the 'Antonine Plague'

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Abstract: This paper responds to recent scholarship by Willem Jongman and Geoffrey Kron that has tried to make a case for elevated levels of prosperity and physical wellbeing in the first two centuries of the Roman imperial monarchy. The relevance of various putative indicators is critiqued. Demographic data as well as anthropometric evidence consistently point to high levels of morbidity and mortality and substantial developmental stress. This evidence is incompatible with an optimistic interpretation of living conditions in that period. The second part of the paper revisits previous arguments concerning the impact of the so-called 'Antonine Plague' of the late second century CE. Papyrological data from Roman Egypt indicate a shift in the ratio of land to labor that is logically consistent with a significant demographic contraction. At the same time, comparative evidence from other periods suggests that the scale of this contraction must not be overrated.

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1. Objective

This paper has two goals: to shed more light on overall levels of physical wellbeing in the population that was exposed to the Antonine Plague (Section 2), and to ascertain the nature and the absolute and relative scale of the epidemics' effect on real incomes (Section 3).[1] The main purpose of this exercise is to provide context and some putative indirect evidence for an event whose actual demographic impact cannot be measured directly.

It is unclear to what extent the death toll generated by the Antonine Plague would have been mediated by general wellbeing: if the epidemic was indeed smallpox striking a virgin population, we must wonder whether existing levels of life expectancy or nutritional status would have played a significant role in determining its scope and force.[2] Those contextual conditions would, however, have affected the speed of demographic recovery. For this reason alone, and given that the precise nature of the epidemic is not quite certain, the issue of underlying levels of wellbeing is nevertheless worth addressing. This has become all the more important as some recent scholarship has sought to portray Roman economic performance and living conditions in that period in increasingly favorable terms.[3] Due to space constraints, I largely leave aside the vexed question of how to track Roman economic growth: several new studies highlight the manifold weaknesses and complexities of potential archaeological proxies for economic development and intensive, per capita growth.[4] Instead, in the following section I discuss more general demographic issues before I touch very briefly on some putative indices of consumption and income (Section 3a).

2. Roman wellbeing

a) Life expectancy

In his chapter in this volume, Geoffrey Kron argues for higher average life expectancy – and thus lower levels of morbidity and mortality – than are usually ascribed to the Roman world. I find myself in agreement with his observation that what professional demographers would accept as "solid evidence" for Roman life expectancy

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Forthcoming in Elio Lo Cascio (ed.), *L'impatto della "peste antonina"* (Edipuglia: Bari).

[1] Section 2 has been written in critical dialogue with Geoffrey Kron's chapter in this volume (as well as with some of Willem Jongman's recent work), for the sake of qualifying and counterbalancing what I perceive to be an overly optimistic view of human development in the Roman empire. Section 3b expands on my earlier work on the economic consequences of the Antonine Plague in Roman Egypt (Scheidel 2002) by addressing a concurrent critique (Bagnall 2002) and incorporating the results of my more recent cross-cultural investigation of early real wages (Scheidel, in press b). I am grateful to Geoffrey Kron for showing me a draft of his paper prior to publication, and to John Sutherland for agreeing to review the papyrological evidence for land rents in Roman Egypt (see below, n.87). I also wish to thank Peter Garnsey and Sheila Ryan Johansson for sharing unpublished work with me, and Saskia Hin for helpful comments.

[2] There is no consensus about the likely impact of a smallpox outbreak even in today's vastly more developed world: see most recently Lucey, Brennan, and Henderson 2009, with references.

[3] Jongman 2007a, b; Kron 2005, 2008, and in this volume. See also Silver 2007.

[4] Wilson 2009a, followed by the debate between Scheidel 2009 and Wilson 2009b. Scheidel 2009, n.3 references earlier literature. See below, Section 3a.

does not exist. Nevertheless, it is important not to throw the baby out with the bathwater: it is certainly not the case that we lack pertinent empirical information. A variety of data sets merit our attention, discussed here in descending order of utility.

Pride of place belongs to the census returns from Roman Egypt in the first three centuries CE. Even though I have shown that Roger Bagnall and Bruce Frier's attempt to match age-specific survival patterns derived from these documents to a single model life table is invalidated by reporting biases that affect urban census returns and information for men more generally, it is nevertheless clear that the census data imply very low levels of life expectancy. Mean life expectancy at birth for female villagers, the only group that appears to be reasonably reliably attested, was most likely somewhere in the twenties.[5]

It is important to bear in mind that this finding is only of limited relevance for our understanding of the demography of the Roman world in general: in fact, given that our data set is heavily dominated by documents from the Fayyum, which appears to have been unhealthy even by the (low) standards of the region,[6] it may not even be representative of conditions in Roman Egypt. It was therefore unwarranted for Bagnall and Frier to extrapolate from the census data to the empire as a whole.[7] In terms of longevity and physical wellbeing, Egypt may well have been located near the bottom of the scale: exceptionally densely populated, hot, annually inundated, and until quite recently a hotbed of both endemic and epidemic disease and consequently subject to very high mortality.[8] There is no good reason to believe *a priori* that the whole empire was like that.

Yet to acknowledge that conditions in Egypt were likely to have been especially poor is not to say that mean life expectancy at birth ought to have been much higher elsewhere, perhaps by as much as a decade or two instead of just a few years. For instance, a long-term survey of genealogical evidence indicates similarly low life expectancy in early China.[9] It is possible that Europe had always been better off than East Asia; then again it may not have been. Ian Morris's new comparative study of development in Eastern and Western Eurasia shows a back-and-forth.[10] Suffice it to note that we cannot take for granted what is in need of empirical substantiation.

Moreover, Roman evidence from outside Egypt likewise points to low levels of survival. What seems to me the most powerful data set covers the Roman elite. As I calculated a decade ago, thirty Roman emperors who are reported to have died of natural causes on average experienced low life expectancy, approximately 41 years at age 19. Following the predictions of standard model life tables, this translates to a mean of

[5] Scheidel 2001a, 142-62, on Bagnall and Frier 1994, 91-110. Scheidel 2001a, 160-1 shows that the data for female villagers match the age distribution predicted by Model West Level 2 Females (for a mean life expectancy at birth of 22.5 years) but notes that due to limited sample size, the observed profile is statistically consistent with a wider range from 20 to 32.5 years (ibid. 175 n.128).

[6] Scheidel 2001a, 175, with 16-18, 82-9.

[7] Bagnall and Frier 1994, 110 ("we are convinced that the returns provide solid support for the emerging picture of life expectancy in the early Roman Empire"). See also Frier 2000, 788-91.

[8] See esp. Scheidel 2001a, 178-9, and more generally *passim* for poor conditions up to the twentieth century.

[9] Zhao 1997, 122, for genealogical data suggesting of a mean life expectancy at birth of *c*.27-30 years in the first millennium CE.

[10] Morris 2010. Moreover, we also find substantial variation within the Chinese data, with mean life expectancies of *c*.37-40 from 1000-1500, *c*.35 years from 1500-1700, and *c*.30 years from 1700-1750 (Zhao ibid.).

around 26 years at birth.[11] The size, structure, and recruitment pattern of the Roman imperial senate and the city council of Canusium in Italy are consistent with a mean life expectancy at age 25 of around 27 to 30 years, implying a mean life expectancy at birth of 25 to 30 years.[12]

I take this opportunity to note that my earlier estimates may now be in need of revision, depending as they do on extrapolation from mean adult survival rates to those at birth, which are empirically unknown. Because it is likely that the algorithm on which standard model life tables are based inflates infant mortality relative to adult mortality, such extrapolation may artificially depress mean life expectancy at birth inferred from death rates later in life.[13] Thus, if we employ the revised high-mortality model life table for 'South Europe' recently devised by Robert Woods, a mean life expectancy of 41 years at age 19 translates to a mean life expectancy at birth of 37 years, fully a decade higher than in my estimate based on the standard model.[14] Given the unavoidable weaknesses of *all* model life tables in predicting the age structure of empirically poorly known high-mortality populations, this discrepancy is not particularly surprising.[15] One model cannot necessarily be expected to yield a 'better' result than another; several models simply create a range that sketches out the limits of the plausible. In this context, it is worth noting that the Woods life table that best fits the age distribution of female villagers in the Roman Egyptian census returns does not imply a substantially different mean life expectancy at birth than the standard model, and that the application of his new high-mortality life table for 'East Asia' does not greatly change the corresponding means inferred from the Chinese clan data mentioned above.[16] This suggests that for all its perceived shortcomings, the standard model life tables hold up fairly well and that the extrapolations of mean life expectancy at birth from adult survival rates that these models support need not be wide of the mark.

If we split the difference between the results for Roman emperors suggested by the two models, Roman elite life expectancy at birth does not rise much above 30 years. This evidence strikes me as particularly important for two reasons. One is that it casts doubt on the notion that access to resources was a critical determinant of physical wellbeing and longevity.[17] A closer look at the causes of death in the Roman elite tellingly reveals vulnerability to infectious diseases.[18] Anecdotal evidence from the later

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[11] Scheidel 1999, 255-6. I ought to note that this finding does not derive from 'anecdotes' but from statistical analysis of 667 years of Roman history. The adult mortality pattern of 17 women connected to emperors fits Model 4 West Females, with an implied mean life expectancy at birth of 27.5 years (ibid. 257-8). For the survival rates of Roman rulers, cf. now also Khmaladze, Brownrigg and Haywood 2007.

[12] Scheidel 1999, 258-65, revising Duncan-Jones 1990, 93-6.

[13] E.g., Woods 1993.

[14] Derived by interpolation from Woods 2007, 379.

[15] See esp. Scheidel 2001c.

[16] The age distribution for $e(0)=25$ in Woods 2007, 379 table 2 is a reasonably good match for the smoothed Egyptian data in Scheidel 2001a, 161 fig.2.24, and his East Asian model implies $e(0)$ of <30 for China in 1-1000, 30-35 in 1000-1500, <30 for 1500-1700, and <30 for 1700-1750, slightly lower in fact than for the standard model (see above).

[17] There is no good reason to assume that Roman elite lifestyle was particularly unhealthy, as it may have been among the "fat-gutted" English nobility invoked by Kron in his chapter in this volume: instead, they would simply have enjoyed even better baths, toilets and other amenities than the general (urban) population.

[18] Zuiderhoek 2002.

Roman empire supports this impression: a comprehensive collection of relevant data is a desideratum. The other reason is that the Roman ruling class was by no means unusual in this respect. In a cross-cultural survey of the mean life expectancy of state rulers in different parts of the globe, I have consistently encountered strikingly low rates if survival even when violent deaths are excluded, and failed to identify demographic benefits of great wealth and status.[19] More generally, the health of elites did not greatly improve until the eighteenth century in England and China, at a time when growing knowledge finally began to enable them to parlay resources into longer lives.[20] All this raises doubts about the strength of the nexus between resources and longevity in the more distant past.

Moving down the scale of reliability, a Roman schedule for calculating annuities known as "Ulpian's Life Table" envisions survival rates that are consistent with a mean life expectancy at birth in the twenties.[21] As we cannot tell whether these projections were derived from empirical data, skepticism is surely warranted: nevertheless, the general impression this text conveys deserves attention, especially when it is placed in the context of other and similarly pessimistic data sets such as those discussed above.[22]

By contrast, demographic information derived from the age distribution of cemetery populations as inferred from skeletons is far less trustworthy. It is well known that such reconstructions frequently produce age-specific mortality rates that are not only inconsistent with any model life tables that are based on or extrapolated from actual historical data but also raise serious doubts about the viability of any population that was structured in such ways. The persistent difficulty of ageing adult bones with precision is an abiding problem: existing methods seem to yield a surfeit of young and middle-aged adults and a grave scarcity or sometimes even complete absence of elderly individuals. While new and increasingly sophisticated ageing methods continue to be developed and debated, they have thus far failed to place the field of 'paleodemography' on a sufficiently solid footing.[23] Moreover, even if these technical problems could somehow

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[19] For a total of 241 rulers (from antiquity or the Middle Ages to 1900) who are not reported to have died of violent causes, mean life expectancy at the mean age of accession was 28 years at age 19 in Spain, 24 years at age 26 in Denmark, 22 years at age 26 in Austria, 24 years at age 22 in Russia, 22 years at age 24 in France, 20 years at age 29 for the Ottomans, 19 years at age 30 in England, and 20 years at age 22 in China. Most of these average reign durations approximate the age-specific mean survival rates predicted by Model West Level 1 Males (with a mean life expectancy at birth of 18 years) or correspond to a mean life expectancy at birth of the order of 20 to 25 years according to Woods' revised life tables (see above). Note that these are preliminary calculations undertaken in the course of my ongoing work on monarchical succession and reign duration in world history (cf. Scheidel 2005) and may be in need of further revision. Zhao, Zhu and Sui 2006 make a similar point about the short reigns of Chinese emperors but unfortunately their methodology is so flawed as to render their paper worthless. I should add that at least in some cases the mean survival rates of rulers are so low that we might have to explore the possibility of clandestine assassinations, a previously unrecognized issue that I will discuss in upcoming work

[20] E.g., Hollingsworth 1977, 327-8; Lee, Wang and Campbell 1994, 401. This is now further supported by forthcoming work by Sheila Ryan Johansson, to whom I am grateful for sharing her work with me in advance of publication.

[21] Frier 1982; other scholars' doubts are referenced in Scheidel 2001b, 20 n.70. Use of Woods' alternative model life table does not change the overall picture.

[22] Cf. also Frier 2000, 790.

[23] Scheidel 2001b, 19 n.66 references the debate up to 2000. For more recent developments, see Hoppa and Vaupel, eds. 2002, and now esp. Chamberlain 2008, 81-132. As I pointed out (*contra* Frier 1983) in Scheidel 2001b, 19 n.68, for every skeletal sample that appears to be consistent with a standard model life

be resolved, we would also need to be able to account for differences between skeletal samples and actual populations that were caused by migration or funerary practice.[24] This is particularly unfortunate given that deposits of skeletons provide a window to the distant past and, at least in theory, ought play a role in demographic reconstructions: if large skeletal samples could somehow be aged with precision and legitimately subjected to stable population analysis, they would answer our questions about local mortality and survival patterns in ways no other body of data could. At present, however, it is simply too soon to decide whether the consistently very low levels of life expectancy implied by skeletal samples are not unduly far removed from reality or whether they are primarily an artifact of forensic deficiencies and representational features. All we can do at present is point out that in order for skeletal material to support significantly higher levels of survival than those conventionally assumed by Roman historians, existing reconstructions would have to be shown to be dramatically misleading. Anthropologists and demographers can be expected to hold different views on whether this is a plausible proposition.

For the sake of completeness, I should add that I am not prepared to put any weight at all on mortality patterns inferred from age distributions in epitaphs, notwithstanding repeated claims that, in different cases, they suggest either high (but sustainable) or catastrophic levels of mortality.[25]

I conclude that the best – or least poor – data we have consistently imply very low life expectancy. This finding was perhaps to be expected for Egypt (given its later poor record), but is more troubling and more difficult to explain away as anomalous when it comes to the evidence for the Roman elite or a generalizing statement such as "Ulpian's Life Table". We also must not forget that powerful indirect evidence of mortality and fatal infection is available in the form of seasonal mortality profiles, a unique source of information that is unfortunately still sometimes ignored in discussions of Roman living conditions. Brent Shaw and I have discussed this material in such detail that only the briefest of summaries is required here.[26] The majority of regional samples – from the city of Rome both in the late Republic and in late antiquity, from North and South Italy, from the Iberian peninsula, from Mauretania Caesariensis (now northern Algeria), and from Egypt, Israel, and Jordan – show that death rates varied greatly between seasons. The underlying causes differed depending on local and regional disease environments. What matters here is that massive seasonal variation in mortality is hard to reconcile with notions of moderate morbidity and wellbeing. The scale of seasonal variability is inversely correlated with longevity: the stronger the fluctuations, the lower life expectancy tends to be.[27] This is relevant especially because the seasonal swings in mortality among the teenagers and adults who dominate the epigraphic samples are

table there are many more that are not. Given a sufficiently large number of cases, fortuitous matches are to be expected.

[24] Cf. Scheidel 2001b, 11, 19-20. Oxygen and strontium isotope analysis has raised our awareness of the scale of migration: see most recently the references to studies of the Roman period in Leach et al. in press.

[25] Scheidel 2001b, 17-19; 2001c, 11-12, 21-22; 2003, 161-2, *contra* Paine and Storey 1999 (catastrophic in the city of Rome); Frier 2000, 791 (sustainable in North Africa). But even Paine and Storey 2006, 82-5 have finally begun to express skepticism.

[26] Shaw 1996; 2006; Scheidel 1994; 1996; 2001b, 1-117; 2003, 162; in press a; forthcoming b.

[27] For a graphic illustration, see Scheidel 2001a, 52, drawing on Sakamoto-Momiyama 1977, 67 fig.4.8 (Japan). Cf. also Shaw 1996, 111 fig.3.

simply unparalleled in the more recent data sets, in which only babies display similarly strong vulnerability to fatal seasonal infections.[28] In the post-Roman period, only the most extreme circumstances produced similar profiles at more mature ages, most notably epidemic outbreaks of plague or smallpox.[29] Cases of muted seasonality are rare in the Roman record, limited in the first instance to late Roman Carthage.[30] This shows that although seasonal mortality surges were not universal, they were by no means confined to notoriously unhealthy regions such as Egypt. Most importantly, they are well attested both in the city of Rome and in peninsular Italy more generally, clashing with claims that privileged access to material resources can be expected to have mitigated mortality. In the case of Italy, malaria and its interacting with other diseases may have been a key factor; elsewhere, in Egypt, Israel, or Mauretania, we simply do not know but must not assume that causation was uniform across different regions.[31] If a variety of environments were capable of generating massive seasonal mortality variation, this phenomenon may well have been of considerable significance for the demography of the Roman world as a whole.

b) Anthropometrics

All the data discussed in the previous section support a very pessimistic appraisal of Roman life expectancy. Skeletal evidence of stature and morbidity does not conflict with this notion. Kron makes much of his observation that the mean adult male body height of 168.3cm computed from Italian skeletons dating from 500 BCE to 500 CE exceeds that found in the same region in the nineteenth centuries or even early twentieth centuries.[32] However, three problems are worth noting. First of all, at 164.4cm, the best estimate of average adult male body height in Roman Central Italy that is currently available is significantly lower.[33] Secondly, because Kron's data are spread out over an entire millennium, his long-term average cannot be related to changes in the level of (in)equality or the provision of infrastructure, which must surely have occurred during this period. This makes it impossible to discern trends and to test for the significance of other factors, such as population size.[34] Moreover, we must not forget that a mean body

[28] See most conveniently Shaw 1996, 119 fig.10; Scheidel 1996, 146-7; and esp. Scheidel 2001a, 39-48.
[29] For examples, see Hatcher 1986, 26 (plague in late medieval Canterbury); Scheidel 2001a, 95 fig.1.41 (smallpox epidemics in seventeenth-century Geneva), 99 fig.1.42 (plague in Cairo in 1801). By contrast, the Roman epigraphic documentation necessarily reflects *endemic* conditions.
[30] Scheidel 1996, 157-61. Alexandria appears to be another example, but the underlying data sample is very small: Scheidel 2001a, 20-1. Gaul/Germany was to heterogeneous for a composite profile to be of much value: cf. Shaw 1996, 129 fig.23.
[31] Malaria: Scheidel 1994; 2003, 163-9. For malaria in Rome and Italy in general, see Sallares 2002. For the possible causes of seasonal mortality variation in Roman Egypt, see Scheidel 2001a, 51-117, esp. 110 table 1.19.
[32] Kron 2005, and in this volume.
[33] Giannecchini and Moggi-Cecchi 2008, 290, a mean that is also consistent with other relevant samples not included in their survey (see Gowland and Garnsey forthcoming). Giannecchini and Moggi-Cecchi 2008, 288-90 argue that the extrapolation method used by Kron is unreliable.
[34] Steckel 2009 discusses the manifold problems of relating human stature to overall wellbeing.

height of 168cm for adult men, even if it could be empirically substantiated, would be historically compatible with very low levels of life expectancy.[35]

Regarding the second problem that I have mentioned, we must take account of the fact that the results of a growing number of studies of skeletal data conflict with the assumption that Roman physical wellbeing peaked in the Republican and early monarchical periods. The most sweeping (and in methodological terms probably most vulnerable) relevant survey has been produced by Nikola Köpke and Jörg Baten. It shows a substantial increase in mean body height right *after* the Roman period, both in the Mediterranean and farther north. Stature subsequently diminished during the high medieval expansion but experienced another surge in the wake of the Black Death.[36] In so far as it can be accepted as representative, this pattern is logically consistent with a Malthusian scenario in which demographic contraction mitigates pressure on scarce resources and survivors are temporarily better off. This survey is highly eclectic, consolidating height extrapolations from bone length that were not derived in a consistent fashion: however, one might note that the same reservation applies to Kron's data set.

It is therefore encouraging that a somewhat more tightly controlled meta-study by Monica Giannecchini and Jacopo Moggi-Cecchi finds that in a sample of 1,021 skeletons from 74 sites in central Italy, body height was greater both in the Iron Age and in the Middle Ages than in the Roman period.[37] Robert Stephan's unpublished survey of 1,867 skeletons from 61 sites in Britain shows strong increases in both male and female body height after the end of Roman rule.[38] All these observations once again mesh well with a Malthusian interpretation. The most reliable results are generated by studies that focus exclusively on femur length, a procedure which avoids distortions caused by diverse extrapolation techniques and ensures direct comparability of different samples. Preliminary findings obtained by Geertje Klein-Goldewijk in her current dissertation project indicate an increase in stature in late Roman Italy, decline in Northern Gaul for much of the Roman imperial period, and long-term stability in Roman Britain.[39] Although her survey has yet to be expanded to include post-Roman material, even these more limited observations are quite telling: they can be read as a sign of growing wellbeing in Italy as population contracted, and of a worsening the more Gaul became 'Romanized'. This appears inconsistent with the notion that Roman infrastructure or egalitarian institutions mattered but fits a Malthusian scenario.[40]

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[35] Adult male height averaged *c.*167cm in Lombardy c.1735-55 (A'Hearn 2003, 371), at a time when *e*(0) (for women) in nearby Piemont and Venetia was around 30 years (Del Panta 1996, 147, for the 1750s-1790s). Adult male height was 167-168cm in France in 1735-1750 (Komlos 2003, 168), when *e*(0) (for women) was 25-30 years (Blayo 1975, 129, for 1740-1790). At the same time, in England a mean height of 168cm in the 1740s-1760s (Komlos 2003, 183) corresponded with *e*(0) of closer to 40 years (Wrigley et al. 1997, 308). This suggests that Roman stature does not tell us much at all about mean life expectancy at birth, which could easily have been as low as usually assumed, i.e., not more than about 30 years.

[36] Koepke and Baten 2005, 77 fig.3.

[37] Giannecchini and Moggi-Cecchi 2008, 290. Paine et al. 2009, 201 provide further matching data, and see also Barbiera and Dalla-Zuanna 2009, 374-375.

[38] Stephan 2008.

[39] I am indebted to Geertje Klein-Goldewijk (Groningen) for sharing these preliminary results with me. It must be noted that further revisions of her database may affect these preliminary findings.

[40] For further discussion, see Scheidel 2009. For the continuing centrality of a Malthusian approach in pre-modern economic and demographic history, see below, n.76. Unlike Kron, I believe that we should worry not so much about potentially misleading applications of Malthusian theory in Roman history but rather

In this context, the apparent increase in meat consumption in late Roman Italy takes on added significance: in so far as the scale of animal husbandry was inversely correlated with population density, and adult stature is associated with protein intake, improvements in stature in late and post-Roman times would seem to make perfect sense.[41] In Italy, the early Middle Ages in particular have more generally been associated with improved nutrition.[42]

This gradually emerging picture of how stature changed over time is difficult to reconcile with optimistic appraisals of conditions in the Roman 'core' period. While comparisons of absolute body height may well make Romans look less immiserated than some populations near the end of the post-Black Death Malthusian cycle or during the Industrial Revolution, the skeletal evidence also suggests that Malthusian forces may have been a more important determinant of relative changes in the medium term than the cultural features privileged by Kron.[43] While absolute body height is important, relative trends also matter.

What is more, osteological evidence of ill-health and chronic malnutrition paints a fairly grim picture, especially for the heartland of the Roman empire, where costly infrastructure, welfare programs, and advanced techniques of food production were probably particularly abundant.[44] The most telling skeletal features are two kinds of bone lesions that are sometimes lumped together but may actually have different aitiologies – namely orbital lesions (*cribra orbitalia*) that are associated with chronic iron-deficiency anemia and other disorders, and porotic lesions of the cranial vault (*cribra cranii*)[45] – as well as enamel hypoplasia, a dental condition resulting from the temporary arrest of enamel matrix growth caused by infection, parasitism, or vitamin D deficiency. While these markers do not enable us to identify specific diseases, their prevalence and distribution are indicative of the overall health status of affected populations.

Preliminary reports on a large imperial cemetery near the ancient via Collatina, about 2km east-north-east of the Porta Praenestina, refer to extremely high levels of enamel hypoplasia (found in 80-92% of individuals) and porotic hyperostosis (77%). This is consistent with findings from more fully published suburban and exurban sites. In a sample from Lucus Feroniae, some 30km north-east of Rome, some 82% of individuals

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about the fact that in the wake of Moses Finley and other cultural constructivists, ancient historians have all too often remained completely oblivious to the importance of the relationship between demographic and economic developments. This situation is only very slowly beginning to change: see now Scheidel 2007a. In this respect, medieval historians are far ahead of their ancient counterparts.

[41] See below, Section 3a, especially n.70. For this correlation, see now Koepke and Baten 2008.

[42] Barbiera and Dalla-Zuanna 2009, 369-371. Cf. Giovannini 2002 for a particularly optimistic appraisal of conditions in the post-Roman period.

[43] In any case, given human susceptibility to density-dependent disease and inequality, instances of an inverse relationship between economic development and stature are not surprising. Shrinkage during the Industrial Revolution is only the best known case. For an earlier example, witness the drop in stature in Northern Europe from the Middle Ages to the early modern period: see Steckel 2004. Mats 2003, 72, commenting on the secular trend of gradually diminishing Dutch body height from the Roman period into the nineteenth century (ibid. 62 table 1), observes that "surprisingly, this dip in stature development coincided with a period of unrivaled economic prosperity in the Low Countries, the so-called 'Golden Age', the age of colonial expansion". It seems likely that earlier inhabitants of that region were taller not because of higher GDP or better infrastructure but thanks to very low population density and a protein-rich diet.

[44] The following draws on Scheidel forthcoming b.

[45] For discussion, see most recently Walker et al. 2009.

and 46% of all teeth exhibit enamel hypoplasia and a third – and two-thirds of pre-adults – had developed advanced *cribra orbitalia*. At Vallerano, about 10km south of the ancient city, 69% had (mostly advanced) *cribra orbitalia* and the incidence of enamel hypoplasia was also very high, at 93% of all individuals, or 64% of all teeth. In a sample from the large cemetery on the Isola Sacra near Portus, the main port of imperial Rome, 81% of individuals suffered from enamel hypoplasia, which was present in 36% of all teeth. At Osteria del Curato, 8km south-east of the capital city, 70% of individuals suffered from enamel hypoplasia. Signs of poor health have likewise been reported for a cemetery at Casale Capobianco near the via Nomentana about 8km north-east of Rome. All these datasets document high levels of ill-health and developmental stress.[46]

Farther afield, at sites in Ravenna and Rimini, *cribra orbitalia* and enamel hypoplasia were found in 56% and 84% of individuals, respectively. In Quadrella in Molise, the incidence of *cribra orbitalia* is relatively modest at 23% whereas enamel hypoplasia affected fully 92% of all individuals and 59% of all teeth. At Urbino, orbital lesions appear on 41% of skulls and all individuals in the sample exhibited enamel hypoplasia.[47]

These findings stand in market contrast to the much lower incidence of lesions in Roman cemeteries from Britain where the incidence of orbital lesions ranges from 5 to 19% and only 5-12% of all teeth show enamel hypoplasia.[48] An intermediate position is occupied by skeletal samples from Croatia, with *cribra orbitalia* on 20% of skulls and enamel hypoplasia in 63% of all individuals (at one site) and 48% of teeth (at another), lower rates than in most Italian samples but higher than in Britain.[49] The only Roman-period site known to me that shows a rate of orbital lesions as high as those in Italy is located in the Egyptian Dakleh Oasis, at 55%.[50] (It catches the eye that the same rate has been found in a modern slave cemetery on Barbados, representing what was presumably a highly 'stressed' population.[51]) By contrast, significantly healthier sites near Rome are extremely scarce: only two appear to be known so far, Casal Bertone, a Roman industrial site less than 2km east of the Porta Maggiore, and Castellaccio Europarco, 8km south of Rome.[52] Moreover, as in the case of stature, conditions in Italy improved after the Roman period: dental and porotic lesions were less common in the medieval period than they had been before.[53]

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[46] Buccellato et al. 2003; Buccellato et al. 2008; Manzi et al. 1999; Salvadei, Ricci and Manzi 2001; Cucina et al. 2006; Nencioni, Canci and Catalano 2001. For a good summary see now especially Gowland and Garnsey forthcoming.

[47] Facchini, Rastelli and Brasili 2004; Bonfiglio, Brasili and Belcastro 2003; Belcastro et al. 2007; Paine et al. 2009.

[48] I here follow the summary by Gowland and Garnsey forthcoming. By comparison, a sample from medieval York shows a much higher rate of *cribra orbitalia*, of 37% (Sullivan 2005), but even that rate is lower than in half of the Roman Italian samples.

[49] Slaus, Pecina-Slaus and Brkic 2004; Slaus 2008.

[50] Fairgrieve and Molto 2000. This is worse than the 45% incidence reported for the medieval Nubian site of Kulubnarti, Mittler and van Gerven 1994.

[51] Corricini, Handler and Jacobi 1985, 702.

[52] Killgrove 2008a, 2008b. Note likewise the relatively low incidence of (mostly mild to moderate) orbital lesions (28%) in a skeletal sample from the Greek colony of Apollonia (fifth-third centuries BCE) on what is now the Bulgarian coast: Keenleyside and Panayotova 2006.

[53] Barbiera and Dalla-Zuanna 2009, 373-375.

None of this bodes well for attempts to attribute comparatively high levels of physical wellbeing to the population of Roman Italy. In their new discussion of the causes of the massive presence of skeletal lesions, Rebecca Gowland and Peter Garnsey observe that since enamel hypoplasia among adults is associated with diminished longevity, the very high proportion of adults exhibiting this condition points to low life expectancy.[54] More specifically, in contrasting the particularly high rates of orbital lesions around Rome and Ravenna (and the Dakleh Oasis) with lower ones in the interior of Italy as well as in Croatia and Britain, they argue for a link between such lesions and malaria, an attractive working hypothesis that demands further attention. What we need is a model that is capable of accommodating not only regional diversity but also age- and gender-specific variation in the incidence of lesions in order to appreciate the complex interaction of nutrition and pathogens in determining health and longevity. A population in which children suffered heavily but adults were not particularly stunted may point to an environment where food intake per se was a smaller concern than exposure to endemic infectious disease that left its mark in the form of skeletal lesions. This in turn casts further doubt on the significance of material resources (such as access to food) relative to more intractable environmental hazards.

c) Nutrition and infrastructure

This leads us to a broader question, that of the relationship between what one might call 'gross' nutrition – what people put into their mouths –, 'net' nutrition – the body's absorption of ingested food –, and longevity. Thomas McKeown's claims about the paramount importance of 'gross' nutrition are by no means generally accepted.[55] Other scholars have, with good reason, put greater emphasis on the role of disease and its links to local ecology.[56]

No one will doubt that clean drinking water is important: a counterfactual Roman world without aqueducts would have been (even) worse. However, we must also allow for the fact that otherwise pure water may become contaminated when it is fetched and stored in containers: this kind of secondary contamination is a serious problems for development projects in the Third World today.[57] How many Romans really had access to fresh water right around the corner, or upstairs, as described by Kron? Roman enthusiasm for 'medicinal bathing' might also give us pause.[58]

But even if we grant that many of the infrastructural features discussed by Kron ought to have been beneficial, we are left wondering about both their scope and their overall impact. Kron's survey focuses very much on urban environments. In a new study of Roman urbanism, Andrew Wilson – certainly no champion of a minimalizing or

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[54] Gowland and Garnsey forthcoming. Cf. also the equivalent observation regarding *cribra orbitalia* and life expectancy for subadults in Mittler and van Gerven1994, summarized in Scheidel 2001c, 9-10 and fig.5.

[55] For measured surveys of the debate, see Harris 2004 and Grundy 2005. Johansson 2005 observes that McKeown's approach has often been critiqued and rejected by historical demographers and economists but was given a new lease of life by its adoption in Robert Fogel's lavishly funded project on mortality history. Kron's reliance on this tradition skews his whole perspective.

[56] See esp. Johansson 1994.

[57] E.g., Clasen and Bastable 2003; Wright, Gundry and Conroy 2004; Gundry et al. 2006.

[58] Fagan 2006 is instructive.

'primitivist' perspective of Roman development – estimates that 10-12% of the population of a Roman empire of 60-75 million lived in cities of 5,000 residents or more, or as many as 14-18% if smaller towns (of at least 1,000 residents) and army camps are included.[59] If true, this means that close to 90% of the total population lived in villages, small rural towns, or in a more dispersed fashion. In quantitative demographic terms, this *was* the Roman world. Living conditions would primarily have been shaped by what went on in those innumerable settlements and not by developments in Rome, Ostia, or Pompeii. Outside Egypt, we know very little about this vast majority, except that it would not normally enjoy access to fancy aqueducts, sewers, and upstairs toilets with a view of Hadrian's proto-Disneyland. It would also have benefited little from welfare projects, which privileged the cities, where the imperial monarchy and urban elites engaged in competitive displays of civic-mindedness.[60]

Town versus country is only one locational divide among several. Others arose from differences in altitude, population density, and climate. Mary Dobson's demographic 'contour maps' for early modern south-east England powerfully illustrate massive differences in life expectancy associated with differences in altitude and concomitant exposure to malaria.[61] Lorenzo del Panta has made the same point for Italy.[62] As Robert Sallares notes, the Younger Pliny already claimed to have observed unusual longevity in Tifernum Tiberinum, an Umbrian town located almost 300 meters above sea level that was described as temperate in the summer and cold in the winter.[63]

Kron's focus on infrastructure and resources privileges one part of the story. In the previous pages, I have sought to redress the balance by focusing on ecological features that stymied human efforts. Very high elite mortality casts doubt on the significance of resources; evidence of extreme seasonal mortality variation and abundant osteological markers of physiological stress seem incompatible with solid levels of wellbeing; post-Roman improvements in stature qualify the Roman achievement. Expensively furnished cities were islands in a sea of rural traditions.

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[59] Wilson forthcoming. A town of 1,000 people would have been smaller than some villages in Roman Egypt.

[60] See most recently Zuiderhoek 2009; and cf. Duncan-Jones 1982, 259 n.1 for civic largesse limited to *intramurani*. While it would surely be unwise to generalize recklessly from Galen 6.749-52 ed. Kühn ("those who live in the cities, in accordance with their universal practice of collecting a sufficient supply of grain to last a whole year, took from the fields all the wheat, with the barley, beans and lentils, and left to the rustics only those annual products which are called pulses and leguminous fruits; they even took away a good part of these to the city: so the people in the countryside … were compelled to use unhealthy forms of nourishment"), the underlying sentiment is nevertheless relevant to our consideration of possible differences in urban and rural provisioning. To forestall facile criticism, I should stress that this is not the same as claiming that all villagers were deprived, which was surely not the case: see De Ligt 1990, 49-55 for a valiant attempt to shed some light on their condition.

[61] Dobson 1997, 224, with 148, 158, 495. Cf. also 178, for the lowest infant mortality rates in the lowest-densities parts of England in 1861.

[62] Del Panta 1989, 22, with Sallares 2002, 160-1.

[63] Sallares 2002, 269-1, on Plin., *Epist.* 5.6.6 and 46.

d) Roman life expectancy?

It will never be possible to average Roman life expectancy. As I and others have already stated elsewhere, substantial variation is only to be expected.[64] For my part, I would expect elevated mortality in warm, high-density, low-altitude areas; I would expect Britain to have been healthier than Italy, Italy to have been healthier than Egypt. I would also expect the majority of the population to have been concentrated in the more disease-prone parts of the empire – in coastal lowlands, along rivers, in areas that favored Mediterranean farming –, with unfavorable consequences for morbidity and mortality. We may safely predict that osteological analysis will continue to throw new light on regional variation in wellbeing. However, unless skeletons can be made to inform us reliably about mean age at death, they will never reveal life expectancy per se.

What emerges from the record is a rather mixed picture: of practices that ought to have been conducive to wellbeing, of better-than-early-modern health or nutrition – features emphasized by modern champions of the achievements of the classical world –, but also of countervailing impressions, which I have deliberately foregrounded here. As I pointed out at the beginning, it is not clear to what extent Roman wellbeing would actually have mattered in the face of a serious epidemic onslaught. Yet in so far as we are prepared to accord some significance to this broader context, it is worth noting that the preponderance of the evidence of morbidity and mortality that is currently at our disposal favors a more muted assessment of overall wellbeing than one might be inclined to infer from the ostensible quality of the food supply and infrastructure in some urban settings.[65]

3. Reconsidering the economic consequences of the Antonine Plague

a) Indicators of economic activity, consumption, and income

Willem Jongman has advanced the thesis that both output and wellbeing peaked during the early monarchical period.[66] If true, this would suggest that the epidemic may have curtailed further development and even caused a subsequent downturn. The archaeological proxies marshaled to support this notion do not, however, hold up under closer scrutiny. Andrew Wilson has forcefully argued for a revision of the chronological distribution of dated Roman shipwrecks from the Mediterranean. Conventionally thought to have peaked in the first centuries BCE and CE, held up well in the second century CE, and dramatically slumped in the third century CE, it now appears that the number of shipwrecks already sharply contracted around 100 CE, a development that cannot be associated with the Antonine Plague. I agree with Wilson that the observed distribution is in the first instance determined by changes in the visibility of cargo that produce a distorted impression of actual trading volume.[67] While an abatement in air pollution

[64] E.g., Scheidel 2001c, 25; Sallares 2002, 283-5. I must confess that I fail to see the relevance of life expectancy around 1900 as tabulated in Kron's chapter.
[65] I return to this question in Scheidel forthcoming c.
[66] Jongman 2007a, b. This period is usually defined as the first two centuries of the so-called 'Principate'.
[67] Wilson 2009a, 219-229, esp. 223 figs.9.3-4 (revising Parker 1992, fig.3); and 2009b, *contra* de Callataÿ 2005; Jongman 2007b, 188-9, and much of the earlier literature since the 1980s. Besides, even if the observed distribution of shipwrecks reflected trading volume, it would point at stagnation in the early monarchical period rather than to a peak in performance: see Scheidel 2009, fig.4.

inferred from analysis of ice cores and lake sediments and the archaeologically documented drop in mining activity on the Iberian peninsula in the late second century CE coincide more closely with the Antonine Plague, it is uncertain how this development affected economic performance, let alone overall wellbeing.[68] Jongman's survey of the temporal distribution of mammalian bones found at Roman sites in Italy and elsewhere produces different peaks for different regions, in the first century CE for Italy and in the second century CE for the empire in general, in a way that makes it hard to relate both of them to a single event determining subsequent decline.[69] More importantly, in the case of Roman Italy, a different survey that focuses on the implied volume of meat consumption instead of the number of bones paints a rather different picture, indicating a slump in the late Republic, intermediate consumption levels under the early monarchy, and a strong increase in late antiquity.[70] Furthermore, a composite graph of mean femur length (as a proxy of body height) based on material from different parts of the Roman empire that shows a maximum between the mid-first and mid-second centuries CE is meaningless because it conceals dramatically divergent regional patterns, not one of which indicates a deterioration around the time of the Antonine Plague.[71]

Jongman's attempt to use slave prices or manumission fees as proxies of overall wage levels likewise fails to support his broader argument. Even if we were to accept that the manumission fees reported at Delphi in the late first century BCE and the few known Roman Italian slave prices from the early monarchical period logically imply an annual 'wheat-equivalent wage' of 1,400 to 1,500 liters for free workers, we would have to bear in mind that this amount barely rises above the annual wheat wage of about 1,200 liters for an unskilled rural wage laborer working not more than 250 days per year that can be derived from Egyptian papyri from the second and third centuries CE and from Diocletian's Price Edict of 301 CE.[72] An annual wage income of 1,400 or 1,500 liters would therefore not have been particularly generous: in fact, the historical record for a number of ancient and medieval societies shows that annual wheat wages of between 900 and 1,600 liters for unskilled wage laborers constitute a baseline range. Genuinely high real wage incomes for unskilled workers, such as 2,000-3,000 liters of wheat equivalent in sixth-century BCE Babylonia or 2,200-3,900 liters in classical Athens, rise far above that level.[73]

We cannot even be sure that extrapolation from slave prices or manumission fees (which are likely to pertain to relatively privileged slaves) allows us to deduce free wages in the first place. What matters is that there is no direct empirical evidence for unskilled

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[68] See Scheidel 2009.
[69] Jongman 2007, 613-4 figs.22.1-2.
[70] Ikeguchi 2007, fig.3-2 (reproduced in Scheidel 2009, fig.3), based on MacKinnon 2004. This effectively cuts the ground from under the enumeration of bone numbers in Jongman 2006, 245-6; 2007a, 612-4; 2007b, 191-3. It also helps put Kron's 2002 arguments about progress in Roman animal husbandry in perspective.
[71] *Contra* Jongman 2007b, 194. See above, Section 2b, esp. n.38.
[72] *Contra* Jongman 2007a, 601-2. For wheat wages in Roman Egypt and the Price Edict, see now Scheidel in press b, esp. table 3, greatly expanding on Allen 2009. Moreover, 1,400-1,500 liters per year approximate 1.7 times the probable mean per capita GDP in the Roman empire (thus now Scheidel and Friesen 2009, 73-4), a ratio that is low by comparative standards (cf. ibid. 72-3, for GDP/wage ratios in pre-modern economies).
[73] Scheidel in press b, esp. table 4. See also below, Section 3b, for elevated wheat wages in Egypt during the early medieval plague period.

free real wages from the core of the Roman empire. Documentary data from Egypt and less reliable references in the Rabbinic tradition point to modest levels of labor income, but of course they need not be representative of other regions.[74] Unless Jongman's conjectures are accepted as indicative of what (from a comparative perspective) would actually have been fairly conventional real wages, the evidence does not support more than a *non liquet* for Italy or the Aegean.

I conclude that the evidence provided by Jongman does not reveal a peak in wellbeing in the century or so leading up to the Antonine Plague. It is important to be precise about this statement: we cannot say that we know that such a peak did not occur, or that it will never be possible to test this claim. It simply means that the evidence so far adduced does not empirically support the case for economic contraction or changes in human wellbeing around the time of the Antonine Plague.

b) The value of land and labor in Roman Egypt

In a paper published in 2002, I drew on documentary evidence from Roman Egypt to demonstrate that economic changes that occurred between the first two thirds of the second century CE and the first two thirds of the third century CE are logically consistent with the notion that a substantial demographic contraction occurred in the late second century CE.[75] The Antonine Plague is the most likely candidate for being the cause of such a contraction. A reduction in population size can be expected to shift the ratio of land to labor in favor of the former, making labor dearer and land and its products cheaper. This should have a beneficial effect on real incomes of workers, although the nature of institutional arrangements and perhaps also the extent of collateral damage would mediate actual outcomes.[76] Relying on data derived in the first instance from Hans-Joachim Drexhage's survey of prices, wages, rents and costs in Roman Egypt, I argued that these consequences can indeed be observed in the papyrological record: daily and monthly wages of unskilled rural workers rose more than the price of important consumer goods such as wheat, wine, and oil, whereas cash land rents and the price of land lagged even further behind, and land rents in kind dropped.[77] In one exceptionally well-documented village, moreover, the total amount of land under cultivation fell between 158/9 and 216 CE while the share of land devoted to arboriculture grew, indicating both reduced demand for basic staple crops and increased demand for non-essential produce such as fruit and wine.[78]

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[74] See, once again, Scheidel in press b, and cf. already Scheidel 2007b, 335 n.51.

[75] Scheidel 2002. The same point has been made much more briefly by Van Minnen 2000, 211-2 and 2006, 162-3, accompanied by an additional claim about the extent of the epidemic in Egypt in Van Minnen 2001.

[76] For the Malthusian-Boserupan dynamics governing the complex relationship between population size and living standards, see Lee 1986a, b, 1987; Wood 1998. Contrary to Jongman 2009, 119-20, Malthusian and Boserupian mechanisms do not represent discrete alternatives historians have to choose from but are mutually interdependent: see esp. Lee 1986a and Wood 1998; and for ancient history cf. now Scheidel 2007a. *Pace* Kron in this volume, Malthusian theory remains the single most powerful theory of pre-modern development: see most recently Clark 2007, 19-189 and Turchin and Nefedov 2009, and cf. also Pamuk 2007; Crafts and Mills 2009.

[77] Scheidel 2002, 101-7, largely based on Drexhage 1991.

[78] Scheidel 2002, 110-1, on Theadelphia in the Fayyum, based on Sharp 1999, 185-9. (Cf. now also France 2000 for the second century CE.)

Inevitably, some of these findings may be less reliable than others. Data sets from different centuries need not be entirely equivalent: the specifics of provenance (from archives or particular locales) may create an impression of difference that need not reflect general conditions. Given the paucity of contextual information about land quality, land prices in particular defy straightforward comparison. Despite all this, in my earlier study I considered the convergence of all of the observed changes a particularly powerful sign that the overall trend is real and not merely an artifact of the evidence.[79]

Roger Bagnall has offered a critical assessment of my argument. Several of his comments do not affect my overall argument and need not be addressed at length. For instance, Bagnall's disagreement with Peter van Minnen concerning the meaning of *P.Oxy.* 66, 4527, a text that may or may not suggest that the wheat tax collected in part of the Arsinoite nome in 184/5 CE was significantly smaller than it had been before the epidemic, has not produced a reliable reading and therefore cannot undermine a model based on a wide range of sources.[80] The sudden doubling of commodity prices in the late second century CE has no bearing on the argument at all: in my comparisons I controlled for this increase by standardizing prices. Moreover, Bagnall's apparent assumption that coin debasement in the late second century CE would have been sufficient to account for this price surge is untenable.[81] Thus, it is hard if not impossible to account for observed price formation without reference to significant dislocations that at the very least coincided – and were therefore probably meaningfully associated with – the Antonine Plague.[82] Bagnall's objection to my observation that wages rose more than the prices of wheat, wine, and oil is misleading. In my study I tracked the relative development of both prices and wages by juxtaposing data from the same two periods, from *c.*100 to the 160s and from the 190s to the 260s CE. It may well be true, as Bagnall points out, that if we average food prices over a much longer period, from the first half of the first century to the 160s CE, and compare them to those after the 180s CE, we will find that they rose more between the former and the latter period than wages rose between the mid-second and the mid-third centuries CE.[83] However, I fail to understand why we would want to set up the comparison in a way that destroys the direct comparability of the data sets in question: the inclusion of prices from the early first century CE, which predate the debasement and increase in the Roman Egyptian coin supply in the 60s CE, would necessarily skew the mean pre-plague prices for these goods downwards. Both the price and the wage data must be derived from the same periods to ensure that we compare like with like. Bagnall's approach fails to meet this precondition.

Looked at more closely, none of these observations can therefore impinge on the model I presented in 2002. Moreover, Bagnall's own survey of land prices establishes

[79] Scheidel 2002, 107.
[80] See Bagnall 2000 and 2002, 115-6 (no effect of the epidemic visible in the text) *contra* Van Minnen 2001 and 2006, 163 n.17 (massive contraction of the tax base). For now Van Minnen, in his latest rebuttal (2006, 163 n.17), appears to have had the last word.
[81] Bagnall 2002, 116. It is true that the Alexandrian tetradrachm was strongly debased around 180 CE but output at the newly reduced standard was too low to have effected a rapid doubling of prices: cf. Christiansen 188, 300.
[82] For the probable reasons behind the price rise (a combination of demographic contraction, an increase in the velocity of coin due to disruptions of credit arrangements, and a gradual increase of the money supply) see now Scheidel forthcoming a.
[83] Bagnall 2002, 119, with (apparent) reference to Rathbone 1996, 332.

very clearly that in most cases the nominal price of arable land did not rise between the pre- and post-plague periods and thus – given the nominal doubling of key commodity prices and wages – lost much value in real terms. As Bagnall himself notes, this finding seems compatible with my model.[84]

This only leaves Bagnall's critique of my use of land rents in kind. He points out the difficulties associated with computing land rents on wheat fields due to the complicated nature of the contracts. He criticizes my reliance on Drexhage's calculations because the latter lists "the *rent per aroura of land planted in wheat*" only, rather than the average total rent per aroura of wheat land over the term of the lease, which would factor in crop rotation.[85] This point is valid in the limited sense that Bagnall's proposed computational method – by averaging the actual annual wheat rent and the wheat-equivalent value of fodder crops grown on wheat land that was not used for wheat cultivation in a given year – reaches a closer approximation of actual lease income per unit of land. Actual income, however, is less important here than the *direction of change* in land rents. If we focus exclusively on the rents levied on land that was actually planted with wheat, it does not matter if that land accounted for all or only part of a given leased plot: all that concerns us here is whether and how rent rates varied over time.[86] The most important thing is that Drexhage's calculations remain consistent across different periods and locations. This is indeed the case: careful review of all the data utilized by Drexhage reveals that he consistently calculates the rent per aroura of land under cultivation of wheat, leaving the qualitative results pertaining to my model intact (Table 1).[87] Moreover, because not all contracts specify crop rotation schemes, the computation of mean annual rents for arable land regardless of whether it was under wheat or not would generate specious values that do not readily lend themselves to meaningful comparison between different periods: apparent changes in mean rents would reflect in the first instance differences in the proportion of contracts that provide for crop rotation in different periods and would be useless for determining variation in rents per unit of land grown with wheat. By contrast, Drexhage's method of focusing on the latter is the only way of ensuring data equivalence across periods.[88]

[84] Bagnall 2002, 117-8. The same is true of "the impression that ordinary land drifts down in value over time, with only those farms in which substantial investments had been made increasing in value" (118). The apparent doubling of the nominal prices for *un*productive land is puzzling (118): however, as most post-plague data points are concentrated in the 180s CE, they may reflect short-term price fluctuations rather than a solid long-term trend.

[85] Bagnall 2002, 119 (his emphasis). He also takes issue with my use of Drexhage's work over Rowlandson 1996. However, the citations were cross-checked in my 2002 article and all documents with a valid (and legible) rent in kind over wheat land were used.

[86] To be precise, for my present purposes – to track the possible effects of the Antonine Plague – it is completely irrelevant whether in the Oxyrhynchite nome between 103 and 165 CE, an aroura of land yielded a mean annual rent equivalent to 5.51 artabas (if land not planted with wheat is taken into account, as preferred by Bagnall) or 7.82 artabas (if only land actually under wheat cultivation is considered, as Drexhage does). The only thing that matters is whether rents – with or without taking account of crop rotation – changed between the period before and the period after the epidemic.

[87] John Sutherland checked all of Drexhage's references against the original publications and recomputed mean and median rents wheat rents for all periods and nomes. Table 1 is based on this survey.

[88] Bagnall's claim that my prior conclusions drawn from Drexhage's data are worthless (2002, 119) is therefore incorrect.

Table 1 Annual rents for wheat fields (in artabas per aroura)[89]

Nome	Period	Cases	Mean	Median
Arsinoite	100-165	34	8.82	7.63
	211-268	19	3.32	3.55
Oxyrhynchite	103-165	12	8.16	8
	205-262	15	5.82	6
Hermopolite	120-161	13	7.47	7.5
	253-268	4	6.27	5.5

For the period from 100 to 165 CE, median land rents are between 7.5 and 8 artabas per aroura for each of the three nomes. For the period from 205 to 268 CE, the respective medians had dropped to 5.5-6 in the Arsinoite and Oxyrhynchite nomes and as far as 3.6 in the Fayyum. The means developed similarly. These findings mesh well with Van Minnen's tally of rents of 8 artabas per aroura in the first and second centuries CE and 6.25 in the third century CE outside the Fayyum.[90] While the specific figures might not capture the exact average rent for wheat fields in each location and time period, their decrease over the given time frame is indisputable and readily fits the overall model.

A graphic summary of the observed changes shows that all factors moved in the predicted direction: labor costs (represented by workers as well as draught animals) rose relative to wheat and even more so relative to non-essential goods, and land values – expressed in prices and rents – dropped even more (Fig. 1).[91]

[89] See above, n.87. These figures are relatively robust. In each region and time period, the difference between the mean and median is small and the percentage drop for each is comparable. (This is less true for the Hermopolite nome due to the small number of cases in the later sample.) There were only a small number of instances where Drexhage's figures needed to be adjusted slightly or a less reliable record needed to be removed for the sake of uniformity. This accounts for the very slight discrepancies between this table and Scheidel 2002, 101 table 1 (now superseded). Note that the third-century figures for the Arsinoite nome could be further modified by *CPR* 1, 33, a document that lists at least 19 different lessees paying rent in kind on wheat fields. Each lessee pays a rate of 2.5 artabas per aroura for plots varying from 1.5 to 9 arouras. Here, this rate is counted once. Counting it 19 times would lower the third-century average to 2.92 artabas per aroura and the median to 2.5, with 37 references overall.

[90] Van Minnen 2006, 173 and again in 2008, 231, unfortunately in both cases without clarifying whether these are means or medians, and without providing sample sizes or provenance beyond the statement that data from the Fayyum were excluded. Rowlandson 1996, 248-9 had already noted a third-century CE decline in rents in the Oxyrhynchite nomes following a peak in the previous century.

[91] Based on the medians in Scheidel 2002, 101-5 tables 1, 3-7.

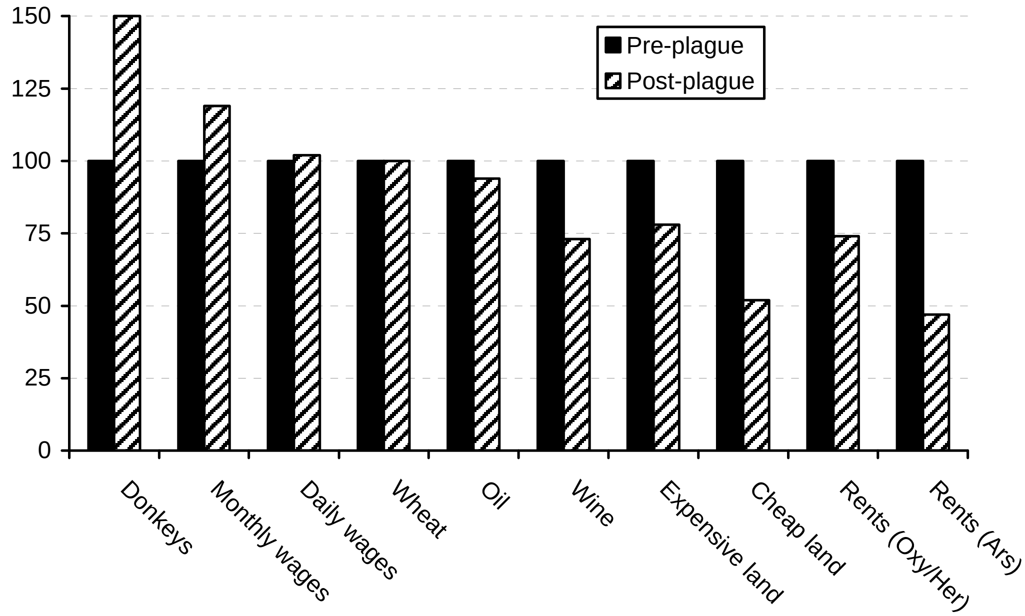

Figure 1 Changes in real prices and rents between 100-160s and 190s-260s CE (all values standardized at 100 for the first period; wheat price standardized at 100 for both periods to reflect the intervening jump in nominal prices)

At this point I would like to add an observation that I had failed to make in my earlier paper. Whereas wages, prices, and rents would have been determined primarily by supply and demand within Egypt, Egyptian wheat prices may well have been significantly affected by demand from outside the region. As Egypt was a major grain exporter, outside demand may have kept prices above the level that would otherwise have been attained. This was likely to happen even if overall demand for wheat outside Egypt had diminished as a result of the epidemic: thanks to high yields, Egypt's competitive advantage in this sector was so strong that exports would not have abated. This helps us understand why the price of wheat dropped less than that of less essential foodstuffs.[92]

Nevertheless, as I already pointed out in 2002, the overall increase in real wages appears fairly modest even if the potential inflation of wheat prices is taken into account. Wheat-equivalent wages rose by between almost nil and close to one-fifth, depending on how they are calculated, and the composite wine/oil wage rose by between one-third and one-half. Moreover, a more sophisticated estimate of changes in the real cost of standardized 'consumption baskets' that seek to approximate total living expenses indicates even less change from one period to the next.[93] Thus, leaving aside the evidence for diminished land value, any implied rises in real wages in post-160s CE Egypt greatly

[92] Moreover, if cash crops had taken over some arable, this would also have stabilized wheat prices and lowered cash crop prices relative to wheat prices: cf. above, n.78.

[93] Scheidel in press b, tables 1 and 2: unskilled rural laborers' purchasing power (measured relative in terms of a 'respectable' consumption style) remained unchanged whereas it improved marginally (for monthly contracts) when measured in terms of a very basic subsistence pattern. For the underlying consumption models, see Allen 2009.

fell short of dramatic improvements such as the doubling of real incomes for masons observed in late medieval England during the century following the Black Death.[94]

This shortfall can be read in (at least) two ways, either as a sign that any demographic contraction caused by the Antonine Plague was much less severe than that precipitated by the Black Death, or as the consequence of institutional arrangements that prevented Egyptian workers from fully benefiting even from a substantial shift in the land/labor ratio. As for the latter, Robert Brenner already argued more than 30 years ago that a purely Malthusian argument was insufficient to account for variation in the observed economic consequences of the Black Death. While Brenner thought that peasant communal power was critical in mediating the effects of a demographic contraction on incomes and living standards, Stuart Borsch's recent comparative study of the effects of the Black Death in England and Egypt shows that the structure of landholding could likewise play an important role.[95] Borsch argues that whereas in late medieval England, plague-induced labor scarcity finished off the manorial system and raised real incomes for workers, the economy of Egypt in the Mamluk period experienced a downturn and falling real wages. Although the actual figures involved are open to debate, Borsch's data suggest that between the first half of the fourteenth century and the second half of the fifteenth century, nominal wheat prices rose in Egypt while the nominal wages of some types of tradesmen fell, resulting in lower real incomes (for these groups) overall.[96]

Borsch conjectures that the Black Death disrupted the Egyptian irrigation system by decimating the labor force that was required for maintenance. Moreover, workers were exploited by the Mamluks, a military aristocracy that formed a class of absentee rentiers who resided in cities (often in Cairo) and maintained only tenuous ties to their estates: rapid redistribution and turnovers were the norm, subject to promotion, demotion, and transfers which were determined by service to the government and shifting political alliances. "Landholding was non-hereditary, short-term, and constantly subject to the winds of political and military fortune. (…) [O]nly newly imported slave children were allowed to enter the military landholding aristocracy. (…) Landed estates were not passed along from father to son."[97] These – historically rare – conditions created a strong incentive to maximize short-term profits at the expense of long-term sustainability. Moreover, relations between the Mamluks and their estates were managed by an intermediate layer of civilian bureaucrats whose main task was the generation of income for the urban rentier class: de facto, for the Mamluks it was these bureaucrats and not their distant estates that were perceived as the source of their income. The Mamluk system was quite collectivist, staging raids of unruly parts of the country to enforce

[94] E.g., Munro 2004. Scheidel 2002, 100 n.22 gathers numerous references to rising living standards after the Black Death.

[95] Brenner 1976; Borsch 2005.

[96] Borsch 2005, 67-90, esp. 90 (GDP), 91-112, esp. 111 (real wages). However, it does not seem to occur to him that the implied annual wheat wages of 430-790 liters for custodians, doorkeepers, water carriers, and readers in 1440-1490, which translate to 1.7-3.2 liters per day at 250 working days per year, are abnormally low and may not even have sufficed for minimal subsistence. See Scheidel in press b, table 4 for a cross-cultural survey indicating a normal range of daily wheat wages of 5 liters +/- 30% (i.e., from 3.5 to 6.5 liters) in a variety of ancient and medieval economies. Munro 2006 critiques Borsch 2005, though mostly with respect to English history, and Pamuk 2007: 300 criticizes Borsch's idiosyncratic use of Egyptian data. Contrast also Ashtor's reading of the late medieval Egyptian data below, Fig.2.

[97] Borsch 2005, 26.

demands for rent. All of this meant that this system would not produce landholders with the inclination, expertise, or financial time horizon to focus on the affairs of their agrarian estates. At the same time, as a class, the landholders had tremendous collective power vis-à-vis the peasantry: "Rather than forming a loose group of individual landholders closely associated with their estates and village communities, Mamluk landholders formed a unified body that would often act together to support their interests in the rural scene, regardless of whose estate or revenue was involved."[98]

In Borsch's view, "collective power and its orientation to the center had vital implications for the dynamics of the system in crisis." More specifically, the Black Death consequently motivated these landlords to raise rents in the face of falling productivity and outputs. Instead of responding to manpower shortages and maintenance needs on their estates with an eye to long-term benefits, landholders pressured the bureaucracy to maintain rent flows. The bureaucrats, in turn, diverted scarce resources to rent payments, interfering with maintenance and repairs. Peasants often fled to the cities or colluded with Bedouins. Bedouins successfully infiltrated abandoned land, which included some of the most fertile parts of the country.

All these developments were quite different from what happened in contemporaneous Western Europe where landholders lacked the collective bargaining power wielded by the Mamluks. Instead, individual bargaining between landholders and workers enabled the latter to bid up real wages. Borsch himself draws a brief comparison with conditions in Roman Egypt after the Antonine Plague, highlighting the differences between Roman and Mamluk institutions. In the Roman period, landholding was predominantly private and hereditary, and owners had close ties to their estates, which they managed individually rather than through a government bureaucracy; they also controlled local segments of the irrigation system. As in late medieval England, they appear to have lowered rents to compete for a diminished labor force.[99]

Given that landownership in Roman Egypt had little in common with Mamluk practices, it seems unlikely that landlords would have been able completely to deprive workers of the benefits of scarce labor: after all, the evidence suggests that nominal wages generally rose more than nominal food prices and land values. Whether Roman Egyptian elites were more successful than their late medieval English counterparts in bargaining for scarce labor is of course a different question; and to what extent increasing state demands for tax revenue constrained private real incomes is yet another. We cannot therefore rule out the possibility that institutional arrangements constrained wage growth in Roman Egypt in a way that masks the actual scale of demographic contraction.

At the same time, evidence for rural real wages during the so-called Justinianic Plague that ravaged western Eurasia from the mid-sixth to the mid-eighth centuries CE may be taken to suggest that the demographic consequences of the Antonine Plague must not be overrated.[100] In 541 CE, the epidemic first appeared in Egypt, supposedly with devastating consequences. Subsequent occurrences are specifically reported for Egypt for *c*.619, 672/3, 689/90, 714/5, 724, 732/5, and 743/4 CE, in addition to more numerous outbreaks in the eastern Mediterranean or "Orient" in general – which includes Egypt – in

[98] Borsch 2005, 33.
[99] Borsch 2005, 18-19. He derives this argument from the information provided by Duncan-Jones 1996, independently of my 2002 article.
[100] For this event, see most recently Little (ed.) 2007 and Rosen 2007.

551/2, 567/8, 573/4, 590/1, 599, and ?607/8 CE, and in the neighboring regions of Syria and Palestine in 542, 580/1, 591/2, 599, 626/7, 634/5, 639, 646/7, 683/4, 687, 698, 699/700, 704/5, 706, 713, 718/9, 725/6, 744/5, and 748/50 CE, events that could easily have affected Egypt as well.[101] These recurrent outbreaks probably depressed population for a couple of centuries.[102]

The economists Ronald Findlay and Mats Lundahl predict that these conditions ought to have changed the ratio of land to labor in a way that raised real incomes.[103] As I have noted elsewhere, this period did indeed witness anomalously high real wages in some parts of Egypt.[104] Figure 2 offers what for most centuries should be understood as a highly approximate time series of daily wheat wages for unskilled (and predominantly but not always rural) workers in Egypt over the course of close to 1,800 years. For the sake of consistency, all means from the seventh to the fifteenth centuries CE are taken from Eliyahu Ashtor's classic study of prices and wages in the medieval Near East.[105]

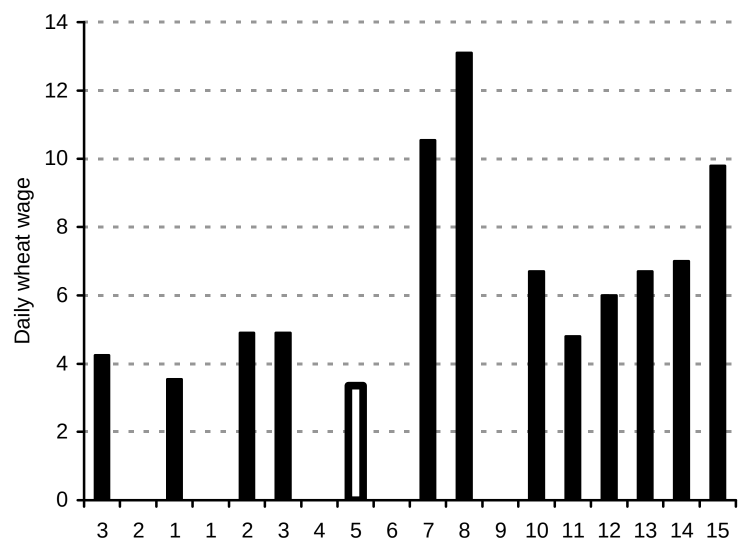

Figure 2 Daily wheat-equivalent wages for unskilled laborers in Egypt, 3rd century BCE – 15th century CE (in liters of wheat)

[101] Stathakapoulos 2004, 278-386 catalogs all known outbreaks. We must also allow for the possibility that the recorded outbreaks are only a sub-sample of all actual incidents.

[102] Compare the considerable scale and effects of recurrent plague outbreaks in early modern Egypt, summarized by Scheidel 2001a, 187-94.

[103] Findlay and Lundahl 2006, esp. 173, 177.

[104] Scheidel in press b, based on Morelli 1996, 1997. The sources for Constantinople and other parts of the Eastern Mediterranean are too poor to shed additional light on this issue: cf. Morrisson and Cheynet 2002.

[105] Ashtor 1969, 465. The earlier values come from Scheidel in press b. The mean for the fifth century (actually for the fifth and sixth centuries) CE pertains more generically to the Eastern Mediterranean. My reading of Morelli's data indicates unskilled daily wheat wages of >7.7 to 13.4 liters from the 580s to the 720s CE, as well as correspondingly higher incomes for skilled workers: Scheidel in press b.

Several features of his graph merit attention. Most wage levels fall within the aforementioned 'conventional' range from 3.5 to 6.5 liters of wheat per day that is typical of most early economies.[106] The focus on daily wages obscures the modest increase of monthly wheat wages in the third century CE noted above. Significantly higher wheat wages are only observed in two periods, during the epidemics of the late Roman and Umayyad periods and – notwithstanding Borsch's claims – once again in the aftermath of the Black Death. Taken at face value, this long-term view suggests that both *Yersinia pestis* pandemics – if that is what they were – had a much more profound impact on the demand for labor than the Antonine Plague. As already noted, the potential influence of institutional constraints precludes a straightforward 'demographic' interpretation of this evidence. Moreover, the probable decline of Egyptian wheat exports in the seventh century CE may have lowered the local price of wheat relative to other goods and services, thereby creating an inflated impression of the magnitude of the growth in real wages in the late Roman and Umayyad period relative to the third century CE. Even so, in so far as the – admittedly scarce – bits of real wage data from the time of the Justinianic Plague are representative of overall conditions, it appears that if what Peter Sarris has called a "seignorial reaction" against workers' demands did indeed occur in the 560s CE, it failed in the long run.[107]

Judging from the documentary evidence for wages, prices, and rents in Roman Egypt in the second and third centuries CE, the Antonine Plague had a non-trivial impact on the real income of rural wage laborers and farm tenants.[108] At the same time, even allowing for exogenously inflated wheat prices and the possibility of detrimental state policies in the third century CE, comparison with the sheer scale of post-plague changes in real wages in later periods of Egyptian history as well as in late medieval England and other parts of Europe speaks against the notion that the Antonine Plague was a truly devastating crisis.[109] In the absence of solid evidence, Roman historians would be well advised to exercise restraint in instrumentalizing this poorly known event as a 'deus ex machina' mechanism to account for presumed demographic or economic transformations.

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[106] See above, n.96.
[107] Sarris 2006, 225-6.
[108] Developments in urban settings remain obscure but can reasonably be expected to have been similar.
[109] This interpretation, which reinforces my earlier reservations in Scheidel 2002, 109, lends a measure of support to critics of aggressive appraisals of the impact of the Antonine Plague, such as Greenberg's 2003 critique of Duncan-Jones 1996 or Bruun 2003, 2007. I should however take this opportunity to register my bewilderment at Bruun's curious insistence on ascribing Duncan-Jones's claims to me as though we had engaged in a joint project (see especially Bruun 2007, 208), a rhetorical ploy that misses the point of my 2002 paper, which dealt almost exclusively with Roman Egypt and merely summarized Duncan-Jones's earlier work in a single – albeit approving – paragraph. I can only hope that the next time I quote some of Mommsen's findings with approval I will likewise be credited with his insights, which would no doubt do wonders for my reputation!

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